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On the "Lower Paleolithic" Site La Belle Roche: An Alternative Interpretation

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given the evident complexity of the site, changes in excavation method, etc.

Thirdly, while rejecting human activities to explain the observed faunal patterns, Binford and Ho employ the same logic as those they criticize. The faunal attribution to man was based upon the belief that the association of stone tools and human remains represented, to a certain degree, a "home" and therefore the fauna comprised food remains. To ascribe the fauna to hyaenas relies upon the assumption that hyaena remains and coprolites associated with *some* gnawed bones represent the product of hyaena denning. But not all the bones are gnawed, and indeed no quantitative data are given as to how many are, so how far can the fauna be attributed to hyaena activity? What significance does one attach to unmarked or weathered bones? These certainly can occur at hyaena dens, as the authors note, but they should not ascribe such bones to hyaena action solely because other bones are gnawed. This would be to follow the same reasoning as taking tools and human remains to represent a home. This problem requires a very detailed study involving the assignment of bone elements to individuals. If gnawed and ungnawed bones can be shown to belong to the same animal, then the authors' arguments will be greatly strengthened. Until then, however, this must remain a problem, for if, as the authors suggest, Beijing man was a scavenger, is it not possible that such scavenging activities might introduce gnawed bone from animal kills?

Thus, when the authors attempt the question "What were the agents responsible for the bone accumulations inside the cave?" they do, I believe, rightly recognize the role of nonhuman actors, a point which is most usefully made but perhaps overstates the role of hyaenas in the light of the available evidence. It is important to note that they use the term "agent" in the plural, so perhaps this paper is a reaction to emphasis rather than argument, although the presentation of full faunal assemblage data, a study of cut marks, and a sorting into "meaningful" units are now highly desirable. Such work probably exists in China, and it is to be hoped that the authors' visit will prompt both further research and wider publication.

An interesting image emerges from Binford and Ho's useful study, one of Beijing man regularly exploiting a known site of hyaena habitation, driving off the occupants and taking their kill. It is to be hoped that such a picture will shortly be testable through work stimulated by this timely piece.

## Reply

by LEWIS R. BINFORD

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The purpose of our paper was to point out the ambiguity of the data reported from Zhoukoudian and to suggest that these data are not easily translated into evidence. When data are ambiguous, little can be inferred with confidence. Nevertheless, our textbooks and scientific papers are full of inferences. We hoped to demonstrate something of the ambiguity, not, as Reynolds implies, to generate new and sound inferences from ambiguous data. The identification of hyena modifications on bones and the old red herring of whether or not ancient hyenas modified bones as modern ones do are really not topics of much controversy at the present time. I will answer simply that ancient hyenas gnawed bones in the same manner as modern ones do, that this is easily demonstrable, and that we do have enough knowledge to recognize this gnawing and bone modification.

We never sought to ascribe all the bones at Zhoukoudian to hyena denning. We never sought to generalize the data from the 1966 work to the entire site: we only used it as an example. Finally, we never employed the same logic we sought to criticize. We did not suggest that hominids introduced no bones, nor did we suggest that hyenas were the sole agents of bone introduction. Reynold's "new" suggestion that hominid-scavenged bone would display animal gnawing simply shows a lack of familiarity with the literature, since I have used this very property to distinguish animal dens from hominid occupations (Binford 1984). Recognizing such distinctions is dependent, however, on statistical information—something that could not be obtained "at a distance."

Finally, the image that is alleged to emerge from our work, namely, one of "Beijing man regularly exploiting a known site of hyaena habitation, driving off the occupants and taking their kill," is certainly in the eye of the beholder.

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The site at Sprimont is being investigated by a team from the University of Liège (Cordy 1980, 1981). The find locality is situated in the limestone quarry "La Belle Roche," where on the right bank of the Amblève River Carboniferous chalk is being extracted. The geological context of the finds has been described by Gascoyne and Schwarcz but will, for clarity, be sketched here too.

The finds come from a horizontal karst gallery, exposed in the upper part of the limestone, which is part of an extensive karstic system situated at ca. 60 metres above the Amblève River. The entire karstic system is filled with detrital sediments and stalagmitic deposits. The horizontal karst gallery is 12 metres long and 1.5 metres high, and two vertical pipes ("chimneys") less than 10 metres high extend to the land surface. The sediment filling of the horizontal gallery consists of a basic gravel unit overlain by a series of mudstone layers, ca. 70 cm thick, sealed by a calcite layer, which Gascoyne and Schwarcz have subjected to U-series dating. The mudstone layer contains rounded limestone cobbles, stalagmite fragments, faunal remains, and, in the upper half, about 40 small pieces of severely weathered flint. In addition to the flint objects, some quartz and quartzite pebbles are present (Cordy 1980, 1981). Gascoyne and Schwarcz mention that the matrix

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by WIL ROEBROEKS

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Gascoyne and Schwarcz's (CA 26:641–42) uranium-series dates for the "Lower Paleolithic site" of La Belle Roche, near the village of Sprimont in Belgium, provide a minimum age of 350,000 years for the faunal and lithic assemblage. The Sprimont site is very important, because only a few archeological sites from this time range seem to be present in northern Europe. The site has been published as a continental counterpart of Westbury-sub-Mendip in Britain, which has been reported to contain archeological material dating from the Cromerian period (Bishop 1974, Bishop 1975, Roe 1981). According to Cook (1983), however, the flint material from Westbury-sub-Mendip does not show clear traces of human working, and we may well be dealing with an assemblage of pseudo-artifacts.

of the faunal and lithic finds must have been deposited as a series of mud flows that descended through the vertical shafts from higher levels in the karstic system, which are now eroded. Cordy has placed the rich and well-preserved micro- and macrofauna, which are present in secondary position, in an earlier part of the Middle Pleistocene because of the presence of *Ursus deningeri* and *Panthera gombaszoegensis*. The U-series dating of the calcite has provided a *terminus ante quem* of 350,000 years for the deposit of the mudstone layer and the objects it contains.

According to Ulrix-Closset (Cordy and Ulrix-Closset 1981), the flint assemblage includes some chopping tools, cores, polyhedra, and flakes that show archaic characteristics and resembles the Buda industry of the Middle Pleistocene site of Vértesszőllős in Hungary (Kretzoi and Vértés 1965; for illustrations of the Sprimont flint assemblage see Cordy 1980, 1981).

I have visited the Sprimont site several times with the excavator, J.-M. Cordy, and have had the opportunity to study the stone assemblage.<sup>1</sup> The purpose of this note is to offer a critical comment about the interpretation of this site and to propose an alternative explanation.

In contrast to Cordy's statement (personal communication, 1983) that the combination of a very primitive flintworking technique and extreme weathering makes identification of the lithic objects as artifacts difficult, I cannot detect any characteristics in the assemblage that can best be attributed to human activities and therefore do not regard the collection as an archaeological assemblage. As Cook et al. (1982:56) have stressed, in these very problematical cases "the burden of proof must fall on the shoulders of the excavator," and the excavators should consider this alternative possibility.

One of the implicit "arguments" for human involvement with the stone assemblage is that flint is not currently present in the Sprimont region. This is incorrect, for the site is in the vicinity of one of the places where the well-known eolith problem was studied. The Belgian geologist Rutot's first and most important eolith site, Boncelles (Rutot 1907), lies ca. 12 kilometres to the west of Sprimont. Oligocene eoliths have been collected in the surroundings of Boncelles, from Liège, to the west of Sprimont, to Baraque Michel, 25 kilometres to the east. According to Rutot and later generations of geologists, this region was originally covered by a *cailloutis*, or cobble layer, that enclosed the eoliths. Nowadays the remnants of this *cailloutis* are known as the (Upper) Oligocene Basal Conglomerate (personal communication, W. M. Felder, State Geological Survey, the Netherlands, 1983; see Calémbert 1954). The "fresh" eoliths collected by Rutot from this *cailloutis*, as well as from higher—Tertiary—levels, are stored at the Royal Belgian Institute of Natural Sciences at Brussels, where I have had the opportunity to study them; the pieces present a general morphology matching that of the weathered pieces found at La Belle Roche (see figures in Rutot 1907 and in Cordy 1980, 1981). Karstic sinkholes may well have trapped early Middle Pleistocene (or older) remnants of this Tertiary cover, which were subsequently transported through the karstic system into the horizontal karst gallery. The Tertiary cover was eroded in later times by the downcutting of the Amblève River, which today flows 60 metres below the level of the site.

This alternative explanation is supported by three observations:

1. Even today, remnants of the Oligocene cover are present at Sprimont and in its environs (Calémbert 1954:515). According to Felder of the State Geological Survey, numerous flints in the *cailloutis* form or as eluvial flint have been found on the right bank of the Amblève River (personal communication, 1986).

<sup>1</sup> I am much obliged to J.-M. Cordy for introducing me to the site and giving me the opportunity to study the Belle Roche material.

2. Rutot (1907:479) also described a case near Fonds de Forêt, 7 kilometres south-west of Liège, where a vertical channel tapped the Oligocene cover and transported several cubic metres of this *cailloutis* inside a cavity.

3. In discussing the site's taphonomy, Gascoyne and Schwarcz note (p. 642) that "neither the faunal assemblage nor the mode of emplacement of the deposits indicates that any part of this cave system was ever occupied by hominids. The presence of the artifacts in the cave sediments may be the result of stream transport or other sediment movement into a karstic sinkhole."

In conclusion, the absence of clear traces of human modification of the Sprimont stone assemblage and an alternative explanation for the "natural" occurrence and morphology of the lithic objects in the karst gallery call into question the interpretation of La Belle Roche as an archaeological site. Minimally, further critical study of the finds and their context is necessary. It is important to discuss these kinds of problems openly in order to keep false information from creeping into the written "archeological record" and being used in other contexts. For example, Schwarcz and Latham (1984:334), reporting on the U/Th-dating evidence from Vértesszőllős, cite Sprimont as one of the sites showing that by the time of the occupation of Vértesszőllős "lithic industries, dominated by large bifacial tools, were already made elsewhere in Europe." The value of the La Belle Roche site lies, in my opinion, in its rich micro- and macrofauna, with a minimum age of 350,000 years, which serves as an important reference for the earlier Middle Pleistocene period.

by DICK STAPERT

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Gascoyne and Schwarcz have provided us with uranium-series dates for the site of Belle Roche in Belgium, which I think look good. It is not my intention to discuss these dates; rather, I want to warn against the suggestion that Belle Roche is a Lower Palaeolithic site, as stated in the title of their report. The excavator of the site, J.-M. Cordy, was so kind as to show me the site and most of the pieces considered to be artefacts (see, e.g., Cordy 1980, 1981) in September 1983. I was impressed by his work, and the Belle Roche fauna is interesting enough. However, I am not convinced that the pieces he showed me are genuine artefacts. More probable in my opinion is that these pieces were produced naturally, by geological processes like soil creep or solifluction (the cave is filled with mudstone derived from outside the cave), cryoturbation, and frost splitting. The problem of (naturally produced) pseudo-artefacts is often underestimated in archeology. Apart from unambiguous artefacts and evident pseudo-artefacts, there also exists a category of pieces of which the artificial character can be neither established with certainty nor excluded. It is in this category ("incerto-facts" would be a good term) that I would like to place the Belle Roche specimens. It need hardly be stressed that such pieces should never be used as a basis for archeological theories.

Comparable problems exist with regard to a number of Dutch sites that are ascribed to the "chopper-chopping-tool complex" by several authors (e.g., Wouters, Franssen, and Kessels 1981, Franssen and Wouters 1983). In my opinion these sites (such as Jabeek, Banholt, and Lunteren) mainly produce pseudo-artefacts or uncertain artefacts in the sense defined above (Stapert 1976, 1979a, 1981a). In fact, I am of the opinion that in the Netherlands we are not yet in possession of artefacts that can be dated with a reasonable probability as older than the Saalian (the penultimate glacial stage as defined in the Netherlands by Zagwijn [e.g., 1973, 1975, 1985], comprising two important interstadials, the Hoogeteven and the

Bantega). As far as we are aware, the oldest sites in the Netherlands are Rhenen (Stapert 1981b) and Maastricht-Belvédère A1, A2 (Roebroeks 1984), which can be dated in the Early and/or Middle Saalian (the artefact found near Moerslag [Stapert 1979b] is considered to be uncertain as far as dating is concerned). Level A2 of Maastricht-Belvédère has a preliminary thermoluminescence date of 200,000–300,000 B.P. (Roebroeks 1984).

I do not feel competent to state a similar opinion on the Belgian material as a whole, but I would like to warn against taking the Belle Roche specimens as definite proof for human occupation older than the Saalian (as defined by Zagwijn) in the Dutch/Belgian region.

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## On the Origins of Image Making

by BRIGITTE DELLUC and GILLES DELLUC

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Like Davis (CA 27:193–202), we do not think that form preceded image and man began by identifying images in the natural forms of rocks. Around 30,000 years ago, in the Aurignacian, at the beginning of the Upper Palaeolithic, someone or some group in the Eyzies region invented drawing, the representation in two dimensions on the flat of the stone of what appeared in the environment in three dimensions. This first drawing was not, however, faithful to nature. Its subjects were, in fact, primarily images of the female genitalia (more than half the drawings). Next in order of frequency come drawings of animals, most often fragmentary—what Leroi-Gourhan has called abbreviations. Images of the male genitalia are rare. Figurative drawings occur alone and in groups. They are sometimes accompanied by series of strokes or little cupules that seem to play, from the beginning, the role of the geometric

signs that are so numerous and so varied on the walls of the subterranean sanctuaries of the Magdalenians and whose symbolic significance escapes us. The Aurignacians seem frequently to have represented the part for the whole, giving evidence of a capacity for abstraction. From the beginning, then, it seems certain that the concept preceded the image.

In the same period, man is capable of making material objects resembling natural models: the phallus of the Abri Blanchard (Aurignacian I, ca. 30,000 B.C.), the animal statuettes of Vogelherd and Geissenklösterle (Aurignacian). The manufacture of these statuettes of bone, the reproduction in miniature of observed reality, represents a different and doubtless less complex intellectual step from the one taken by artists transcribing on rock surfaces the images of subjects observed in nature in three dimensions.

It is worth pointing out that parallel incisions occur earlier than the Aurignacian on Chatelperronian ornaments (for example, at Arcy-sur-Cure), but since they are not associated with figurative drawings it is difficult to say whether they are images or play merely a decorative role.